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but this requires the hypothesis of a constant error in all the observations, amounting to + 1°·3. Without any such hypothesis, the inclination comes out 2°·9, but in this case the probable error of the observations is much larger. It is obvious that either result is entitled to very little weight, the probable error in any case being very large.

If the Zodiacal Light is a solar appendage, we should not be surprised at finding its axis in the plane of the solar equator; but it is quite evident that such is not the case. We should have no reason for expecting it to be in the plane of the ecliptic rather than in that of any other planet's orbit; it would be more likely to be in the plane of the average position of the orbits of all the planets. But I have not calculated what this plane is, so as to avoid every bias in my investigations as far as possible.

Except on two nights, all the observations were made at Sunderland, lat. 55° N.

I have been aided in the calculations by J. Taylor, E. Dodshon, A. Gröne, and A. J. Edmunds.

Sunderland:
1881, *April* 27.

Note on the Flexure of the Greenwich Transit Circle.
By W. H. M. Christie, Esq.

Prof. Newcomb has recently called my attention to the practical difficulties attending the determination of the astronomical flexure by means of opposing collimators, and to the effect which the increase in the temperature of the air from the bottom to the top of the observing room might have on this observation. And he points out that if no change had been made in the adopted flexure at the time of the piercing of the cube of the Transit Circle, there would have been no striking change in the R−D correction. In his work on the "Declinations of Fixed Stars," pp. 35 and 36, Mr. Lewis Boss has examined the flexure and R−D corrections 1851–1872 with reference to Prof. Newcomb's suggestion, and has found that if the uniform flexure $+0''\cdot21$ had been employed throughout the series, we should have had sufficiently good agreement between direct and reflection observations. This view entirely agrees with the conclusion at which I arrived in my paper on the Systematic Errors of the Greenwich N.P.D.'s from the discussion of the observations of circumpolar stars and of the Sun, before I was aware of Prof. Newcomb's suggestion or of Mr. Boss's work.

As further data are now available, I have thought it might be not without interest to compare the flexure and R−D corrections for the period 1851 to 1880. The following table is derived from that which I gave in the Addendum to my paper

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(*Memoirs*, vol. xlv. p. 182) with the addition of the R—D correction for 1880—viz.

$$+0''.07+0''.54 \sin z \cos^2 z,$$

which has been recently computed.* I have converted the corrections from the law $b' \sin z \cos^2 z$ to $b \sin z$ by multiplying b' by ·843. The R—D correction is thus the correction apparently required to the Adopted Flexure.

	R−D $b \sin z$.	Adopted Flexure.	Apparent Flexure.	Weight.
1865	−0″·65	+0″·76	+0″·11	1
1861–64	−0·45	+0·56	+0·11	4
1857–60	−0·39	+0·56	+0·17	4
1851–56	−0·29	+0·58	+0·29	6
1879–80	+0·39	0·00	+0·39	2
1871–78	+0·43	−0·12	+0·31	8
1866–70	+0·65	−0·37	+0·28	5

Under the head "Apparent Flexure" is given the flexure correction indicated by the reflection observations of stars, found by adding the coefficient of the R—D correction to the Adopted Flexure.

Taking the means of the Apparent Flexure for the whole period and for the periods before and after the piercing of the cube, we have—

Mean Flexure	1851–1880	+0″·25
„	1851–1865	+0·20
„	1866–1880	+0·31

From this it would seem that if any change of flexure occurred on the piercing of the central cube of the Transit Circle, it was in the opposite direction to that indicated by the Collimator determinations, and was simply a small increase in the coefficient. With regard to the apparent change of sign indicated by the collimators, it is to be remembered that before the piercing of the cube the observations were made with collimators of four inches' aperture and with the instrument raised, and that after the piercing they were made with collimators of seven inches' aperture and through the openings in the cube. Now it is found that in the determination of collimation-error there is a discordance between the readings taken with the instrument raised and through the cube respectively, and this difference affects the result for collimation-error in the mean by 0″·30. If abnormal refraction resulting from inequalities of temperature has anything

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* The Adopted Flexure for 1851 and 1852 was really 0″·73, instead of 0″·50. I have here corrected for this error.

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to do with this discordance, it seems not unnatural to suppose that it might produce even a larger effect in zenith distance. No such discordance, however, has been detected, though some observations with the instrument raised, and through the cube, have been made for this purpose.

The recent determinations of flexure by the collimators do not, however, differ much from that indicated by the R−D correction. The following are the results since the beginning of 1879:—

1879, April 22	+0″·26	1880, July 22	+0″·04
23	+0·07	Dec. 6	+0·20
1880, April 6	+0·19	1881, May 2	+0·13
May 6	+0·04	May 10	+0·18

The mean of these is +0″·14, whilst the mean from the R−D correction is +0″·25.

With reference to the question of the distribution of temperature in the room, the following readings of thermometers mounted respectively near the north and south collimators and the central cube, as well as of the exterior thermometer, were taken on 1881 May 4 and on May 10 at the beginning and end of the observations:—

	N. Coll.	Cube.	S. Coll.	Exterior.
h	°	°	°	°
May 3 22	49·6	49·8	49·8	50·8
9 22	47·8	48·3	47·5	47·9
9 23	48·6	48·9	48·6	49·1

The readings of the thermometers were inadvertently omitted on May 2; those on May 4 are given as illustrating the distribution of temperature under nearly similar conditions. Considering the difficulty of determining the true temperature of the air in the neighbourhood of large masses of iron and stone, the readings of the interior thermometers seem to correspond as closely as can fairly be expected.

In conclusion, I must point out that although the R−D discordance may reasonably be explained as the effect of error in the flexure determination, there yet remains to be accounted for the error of nearly 1″ at Z.D. 60° North, which is indicated by the circumpolar observations 1868–1876, and this is to my mind a much more serious matter.

Blackheath:
1881, *May.*